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PROBLEMS OF INFERENCE IN THE SOCIO-PHYSICAL SCIENCES

IT is a widely accepted view that there exist two fundamental classes of sciences, the physical sciences and the social sciences, whose characteristics can be well defined and distinguished. Very little attention has been paid, however, to a composite class of sciences which intersect in a fundamental way both the physical and the social sciences and which may therefore be called socio-physical sciences.

An important and typical example is industrial engineering, which has widely but erroneously been considered a physical science; this belief is ultimately responsible for the fundamental weaknesses that have been found to exist in, say, the classical work measurement component of industrial engineering. The fact is that industrial engineering and other socio-physical sciences are concerned with the planning and control of a complicated phenomenal system where both physical variables and human variables are crucially involved.

The key role of the human variables means that the socio-physical sciences must be vitally concerned with human behavior and objectives. To give an extremely simple example from industrial engineering, an optimal machine design is not one which considers only mechanical factors, but one which also takes into account the operator's characteristics. A more complicated example is planning and controlling the interdependent activities of machines and men so as to optimize some utility function defined in a wide sense for a given industrial organization. Such an optimization must take into account the needs, abilities, and goals of the individuals and social groups involved in the organization, including those of the organization itself.

As these examples suggest, the class of socio-physical sciences did not acquire distinguishable characteristics until modern industrial technology had had its full impact upon contemporary civilization. Many areas classically considered within the realm of the social sciences then began to exhibit the characteristics outlined above with respect to industrial engineering. Industrial sociology, industrial economics, and industrial psychology are three related

and suggestive examples. Likewise, many problems in economic theory, logistics theory, organization theory, and communication theory may profitably be considered as socio-physical problems.

As the impact of the industrial technology becomes even more pervasive and all-encompassing, it is to be expected that many more areas, some as yet undefined, will fall into this class. Thus, the eventual extensive use of automation will bring into the class many additional areas now considered components of the social sciences as well as some now considered components of the physical sciences.

In sum, the necessity for considering a technology directly in terms of the goals of the social groups involved in or affected by that technology is what ultimately distinguishes this class of sciences from the other two classes. Additional and more specific differences are brought out in the body of the paper, where they are shown to have a profound effect on the process of making inferences.

The Role of Formal Methods in Making Inferences

Although they were originally primarily experimental sciences, the physical sciences have achieved their present state of near-arrogant sophistication largely because of the periodic invention of theories from which important inferences could be made by formal methods. In fact, progress in many of these sciences—and physics is a prime example—would have been greatly impeded, if not completely inhibited, had there not been periodic mathematical developments to solve the equally periodic dilemmas of the experimenter.

In fact, some of these mathematical developments have dramatically pointed out the existence of relationships which were ultimately verified only after a painfully slow and a painstakingly exhaustive amount of experimental work. This is not to say that physics, for example, has slighted the experimental method. The great progress made in this field is ultimately due to the continuous and sequential coupling of formal and experimental results. But, particularly in the area of microphysics, so many recent results have been from the realm of mathematics that experimentation seems to have been relegated to the relatively apologetic role of verifying relationships it almost dares not deny. This is why Russell says of mathematical physics that it "contains such an immense superstructure of theory that its basis in observation tends to become obscured."[1]

[1] Russell, Bertrand, *Human Knowledge: Its Scope and Limitations*, Simon and Schuster, 1948, p. 27.

The success of formal methods in mathematical physics is certainly not accidental, nor is it due to the unique analytical skills of mathematical physicists. Actually, the formalizations in this field have been developed from a long and rich background of prior experimental and formal results. Another and decisive factor in this success is the inherent stability of physical phenomena which leads to quasi-deterministic relationships at the macrophysical level and to probabilistic relationships at the microphysical level.

The probabilistic relationships developed at the microphysical level are almost exclusively concerned with large aggregates of atoms rather than individual atoms. This development has been accompanied by philosophical statements indicating that it is sufficient in this area to be concerned solely with functions of large aggregates of individual atoms. Boltzmann says, for example, that "there is no point in pursuing the motion of each individual atom or molecule; it is only important for us to observe the average statistical behavior of large numbers of atoms."[2] This position is particularly attractive in view of the apparently insurmountable difficulties involved in attempting to characterize the behavior of individual atoms. Since there is no direct operational method of verifying or rejecting this position, one must turn for guidance to the history of science; this history makes it quite clear that there always *is* a point in obtaining more information about any given set of phenomena.

In any event, the Boltzmann position can be defended in the following qualified sense: it seems sufficient *for present empirical purposes* to consider only functions of large aggregates of atoms. There are two interdependent reasons for this: (1) amply large aggregates of atoms are readily available; (2) the aggregates developed seem to act like homogeneous statistical populations with well-defined and stable probabilistic properties. This makes it possible to apply mathematical statistics theory to these aggregates and obtain generalizations that seem sharp enough for present empirical purposes. If aggregates of atoms did not have the required properties, however, physicists would have to face the problem of characterizing the behavior of individual atoms for which no formal theory has yet been constructed.

Except possibly for aesthetic considerations, the remarkable success of formal methods in the physical sciences explains why it has become extremely fashionable in the socio-physical sciences. The result has been what seems a decided emphasis on formal methods, accompanied by a seeming impatience with the need for

[2] Boltzmann, L., as quoted by Jordan, P., *Physics of the Twentieth Century*, Philosophical Library, 1944, p. 62.

experimental work. Also, the formal models considered are usually all-encompassing, suggesting that it is of the utmost urgency to solve a problem in complete generality before its particular characteristics are understood.

As the foregoing material suggests, the concentration on formal methods will not be nearly so successful in the socio-physical sciences. One of the reasons for this is that there has not been an adequate background of preliminary experimental and formal results. In fact, there seems to be a definite reluctance to consider certain experimental evidence already available. The result is that in most cases the only intersection between the formal model and the empirical situation is in the definition of extremely broad classes of entities by the process of aggregation.

The ancestor of this process of aggregation is the process of aggregation found so expedient by physicists. In the socio-physical sciences, however, it is difficult and often impossible to find large numbers of entities with essentially the same properties. This has led to the formation of aggregates made up of entities with sufficiently dissimilar properties so that the aggregates are neither homogeneous nor stable in their properties. As an example, labor is defined as an aggregate of workers in certain linear programming and other formulations. Considered naïvely, the definition makes it appear that labor is a homogeneous collection of individuals with similar occupational properties, although this is clearly not the case.

One of the difficulties here is that the process of definition is too often taken to imply that the thing defined has the properties imputed to it by the definition. The fact is, however, that definitions are actually inferences, probably the most important in the entire inferential hierarchy. Their validity must therefore be evaluated experimentally where, as in the case of labor, many of them turn out to be operationally invalid.

The labor example illustrates another fundamental and related reason why concentrating on formal methods will not be nearly as successful in the socio-physical sciences as it is in the physical sciences. The aggregates formed frequently do not have the properties required to successfully apply formal methods, probabilistic or otherwise. This means that the behavior of individual members of the aggregates cannot be ignored, since it is because of their differences that aggregation fails to be productive. To complete the fundamental cliché of the so-called industrial relations expert with reference to workers: *it is precisely because each worker has unique occupational properties* that he must be given individual attention.

Also, the application of formal methods in the socio-physical sciences is often preceded by certain extremely restrictive assumptions about the nature of the phenomena considered. Perhaps not unlike certain physical science analogues, these assumptions are sometimes intended only to simplify the analytical aspects of the problem.

The overall result is that the mathematical solutions obtained—even in the limited number of cases where they are economically or physically feasible—have little, if any, direct problem-solving value. Instead, the primary value of the formal approach in these cases is its ability to represent an empirical area in concise symbolic form. This helps give the investigator clues on how to organize experimental studies, providing him with a knowledge of what classes of problems can be solved and under what conditions. It also provides him with specific concepts which can be extremely useful in treating experimental data such as, for example, the expected value concept of mathematical statistics theory. In the present stage of development of the socio-physical sciences, then, formal methods can be said to provide existence theorems for the solution of quasi-abstract problems; it remains for experimental work coupled with further formal work to provide the corresponding constructive theorems for the solution of phenomenal problems.

The Key Role of Purposive Behavior

By now, it should be clear that one of the most critical problems faced by the socio-physical sciences is characterizing human behavior effectively. However, the problem is far from hopeless, for there exists a respectable amount of information on this subject. As indicated above, for example, there is considerable evidence that phenomenal systems involving human behavior are not inherently stable in the sense of being amenable *a priori* to formal analytical treatment.

The primary reason is that human behavior includes and can largely be characterized by its purposive component. This purposive behavior is unique to the individual and is based on past events and behavior as well as on anticipated events and behavior. This process is, of course, a feedback process but an extremely complicated one, introducing a decisive planning and control element into the phenomenal system involved. It is the uniqueness of this behavior with individual human beings that defeats the process of aggregation and, ultimately, the process of formal analysis. Unlike the phenomena in macrophysics, then, the effects of purposive behavior are certainly not deterministic; unlike micro-

physical phenomena, its effects inhibit a straightforward probabilistic interpretation.

Looking more closely, an individual's behavior is continuously interacting with that of other members of the immediate environment. Also, the purposes motivating this behavior are fixed neither in number nor in weight, for purposes may themselves change with past and expected behavioral history. In industrial organizations, purposive behavior exists at many different levels and represents many different simultaneous and overlapping groupings. In effect, there exists a network of sometimes conflicting, sometimes complementary purposes that are always interacting and have a varying relative importance. A great deal more can be said about this behavior with reference to its impact and role in specific theories, such as work measurement theory.[3] It suffices here to say that sharp solutions of problems in the socio-physical sciences will not be forthcoming unless purposive behavior is understood and taken fully into account.

Inference-Making in Socio-Physical Sciences

In socio-physical science problems, the initial need is for a fundamental body of experimental evidence. Particular attention should be paid to human behavior, both as formally defined and as informally exhibited. This precept underscores another problem of some consequence in definitions involving human behavior: the formal codes governing that behavior are often intended to satisfy some criterion of social acceptance rather than to reflect actual behavior. It is also necessary to observe this behavior at many levels of aggregations, probing as far into microbehavior as the currently available observational and analytical techniques permit.

It seems also that there exists a lower limit on feasible levels of observation, one for the purposive aspects of a given problem and another for the non-purposive aspects. These lower limits are, of course, a function of a number of parameters, such as the observational and analytical techniques available. In fact, it may be necessary to abandon the study of microphenomena at a level originally thought to be fruitful and work at a level of higher aggregation. The ultimate reason is the same as it seems to be in microphysics; aggregation is needed to obtain either observationally tractable or analytically tractable results.

The fact is that it is impossible to define certain variables unless this is done. Once again, one finds an analogy in the

[3] Abruzzi, A., *Work Measurement: New Principles and Procedures*, Columbia University Press, 1952, Chapter 2.

microphysics case where Bridgman, among others, insists that certain subatomic entities exist in the mathematical world rather than in the phenomenal world.[4] The corresponding situation in socio-physical problems arises because the verbal definitions of certain entities seem to give those entities properties, including existence, which turn out to be illusory upon close examination.

This is especially likely to be true of those definitions which involve a formal extrapolation from a certain level of aggregation, where the variables defined do have existence and certain properties, to higher or lower levels of aggregation where they may not have the same properties or even existence. This is one of the difficulties with definitions like the labor aggregate in which the problem occurs with an extrapolation from the individual worker to a higher level of aggregation. A similar difficulty occurs with, say, definitions of operation subdivisions in work measurement theory, which become meaningless when an extrapolation is made from the individual operation to certain lower levels of aggregation, say, individual hand motions.[5]

Such situations are, of course, well known in physics where one has no difficulty in defining velocity and position as independent variables at the macrophysical level. The extrapolation of these definitions to the microphysical level, however, gives rise to the great difficulties summarized in the uncertainty principle. The analogous difficulties illustrated above demonstrate that there is also an uncertainty principle in the socio-physical sciences. Because it takes a different form with each class of problem, it can only be phrased in descriptive terms: *given the present state of observational and analytical technology, certain entities do not have the properties, including existence, imputed to them by their definitions.*

Unlike the uncertainty principle of physics, however, this principle may apply to extrapolations involving a relatively high order of aggregation as well as to those involving a relatively low order of aggregation. The fundamental difference between the two cases is in the impact they have on the imagination. It is intuitively simple (and perhaps emotionally satisfying) to accept the notion that a lower limit has apparently been reached in the ability to observe or to analyze meaningfully. But if the second case is intuitively simple, the first is analytically subtle. The collection of workers into the labor aggregate, for example, also seems intuitively simple and readily acceptable. It takes considerable probing and analytical skill to look into such a collection and

[4] Bridgman, P. W., *The Logic of Modern Physics*, Macmillan, 1927.

[5] Abruzzi, *op. cit.*, Chapters 9–11.

understand why it fails to be a determinate entity with essentially stable properties.

This provides a natural background for commenting that, in the socio-physical sciences, standards on what can be considered empirically adequate levels of observation are continually changing, if only because of changes in observational and analytical techniques. Thus, if it were possible to characterize human purposive behavior in much sharper terms than at present, a use would immediately be found for the additional information available. At least in these sciences, then, the indeterminacy principle is not an unconditional principle but rather a direct function of the current technological state of the field. Looking backward, certain variables now characterized as random variables within a probabilistic framework were considered indeterminate prior to the development of the appropriate mathematical theory.

This example also points up an ever-present danger in inferential processes. The development of a new branch of mathematics is seized upon and used by investigators in many fields without a thorough determination as to whether the phenomena they are investigating meet the postulates and assumptions of that mathematics. This is a contagious practice whose rate of propagation seems to depend on the number of those who have already contracted the infection. This situation seems currently to be the case with mathematical statistics, which is sometimes used to generate what are taken to be precise statistical estimates regarding certain phenomena which have not even been shown to be measurable in numerical terms.

Even under optimum circumstances, however, the imputation of, say, probabilistic properties to certain phenomena is quite subtle. It is always tempting to resolve a basic indeterminacy by considering the problem in probabilistic terms. Although a real attempt may be made to determine whether this is justified, the ultimate legitimacy of such a procedure must be evaluated by the classical operational process of determining whether it brings about a greater degree of predictability. A decisive negative criterion, of course, is the development of a superior respresentation for the phenomena in question, in which case the earlier representation is discarded.

The preceding material strongly suggests that, especially in the socio-physical sciences, what is sought, how it is sought, and what it is thought to be depends ultimately on the attitudes and objectives of the investigator. Since an investigator's behavior is quite as purposive as the behavior of other human beings, these are

greatly influenced by the attitudes and objectives he inherits from earlier investigators in the field. All too often, this takes the form of intellectual ancestor worship where many generations of investigators find themselves in a sterile search for minutiae that can be used to support an equally sterile but fashionable hypothesis. Most doctoral investigations appear to fall into this class and, ultimately, find a well-deserved place in obscure library stacks to be consulted only by those working on similar investigations destined for the same fate. These remarks are a negative way of saying that the significance of an investigation is a direct function of the ability of the investigator to free himself from the inhibitory aspects of historical attitudes and objectives.

Now, there exist areas in which an investigator's objective is not in generalization but in describing an essentially unique set of phenomena as sharply as possible. In that case, observations are made at the most minute possible level of verbal description since there is no problem of formal analysis. This might be the case, for example, in clinical psychiatry. The areas involved roughly encompass what are usually called the arts where the emphasis is on richness in describing a local and unique situation. Another way of saying this is that the experimental process of aggregation and the analogous formal process of generalization have only a secondary function in the arts.

In the physical sciences, however, the professed objective is not to describe local situations, such as the behavior of individual atoms. Rather, the objective is taken to be the discovery of generalizations applying to large aggregates of atoms. Thus, the richness that might have been obtained by considering individual atoms —assuming it were possible to obtain meaningful results at this level—is ignored in the interest of describing somewhat less well a large collection of atoms.

This distinction provides a plausible way to express the essential difference between the arts and the sciences. In the arts, primary interest is in uniqueness with regard to the empirical situation and in richness in characterizing it. In the sciences, primary interest is in aggregation with regard to the empirical situation and in generalization in characterizing it. This is not to say that a sharp division exists between the objectives of the arts and of the sciences. In fact, the socio-physical sciences must combine both objectives to be effective. The frequent failure of workers in these sciences to arrive at meaningful results can largely be traced to their considering only one or the other of these objectives.

Designing a Fertile Investigation

In making experimental studies, it is considered useful to establish certain rules of procedure, which are sometimes called postulates of experimental inference. Exploring these rules in any depth would require an exhaustive treatise. Even then, the result would be extremely unsatisfactory since at best it would yield a set of rather barren qualitative statements cataloguing the steps intuitively followed by any experienced investigator.

A much more useful exploration would be concerned with how the rules can be implemented so as to design a fertile investigation. This is an extremely difficult problem, since the decisive component of successful experimental work is unique to the investigator and cannot be characterized by a verbal description. This so-called "intuitive" aspect of scientific work means, among other things, that one is forced to take the rather paradoxical position that the scientific method is fundamentally an art.

This also explains why it is only after a scientific investigation has been successfully completed that a logical structure for the investigation can be constructed from the diffuse notes of the investigator. The logical structure, then, is appended to the investigation not because the investigation itself followed a neat scheme of logical steps but because the investigator and the scientific community impose the largely aesthetic demand that an investigation should be logical. To take an extremely simple example, mathematicians always set down their assumptions at the beginning of a paper although the necessity for many of these assumptions is discovered only after considerable investigation.

In sum, there is no theory on how to develop theories; one can obtain guidance—and it is a limited amount at best—only by observing how a given theory was constructed. It might be remarked parenthetically that somewhat more guidance might be obtained if successful investigators were to publish the actual path taken by their investigations and the reasons why they took the path they did. Thus, it would seem more helpful to subsequent mathematical investigators for a mathematical paper to say, for example, that a given assumption became necessary in order to insure the convergence of a certain function than for it to set down that assumption without comment or explanation.

Defining the Goals of an Investigation

A primary objective of making experimental studies is to achieve as sharp a degree of precision or predictability as possible for as wide a class of phenomena as possible. Stated in such broad terms,

this goal is common to all sciences, since it is the consensus of society that precision is a necessary condition for making meaningful empirical inferences. This social consensus means that precision is implicitly a socially desirable goal accepted by the entire society.

As indicated above, the nature of physical phenomenal systems means that the physical scientist is not concerned with the social problems created by, say, the necessity for directly considering human behavior. Consequently, he *need* not consider social goals explicitly except insofar as the goal of precision is a social goal. The result is that he can devote himself to the relatively restricted methodological problem of achieving that goal. An unfortunately large number of physical scientists have concluded from this that theirs are the only "true" sciences; the fact is that theirs are the only sciences in which the problems are so simple as to be purely methodological.

On the other hand, the socio-physical scientist cannot confine his attention simply to the goal of precision. He must also be directly and intimately concerned with optimizing a utility function explicitly taking into account the goals of the individuals and social groups crucially involved in the systems he considers. The goals in question, moreover, are variable rather than fixed, always interacting, sometimes complementary, often contradictory, and generally vaguely defined. These goals also change in number and weight from one local environment to another. This means that the goal of precision in the socio-physical sciences is not an unrestricted goal; it is restricted by the need to take into explicit account the complicated purposive behavior of individuals and social groups.

These notions can be made more concrete by considering the problems of work measurement. If one were to approach these problems from the viewpoint of the physical sciences, he would simply develop a methodology yielding a maximum amount of precision. Ultimately, this would require a drastic change of workers' purposive behavior to the point where they would cease to be purposive at all, at least at the workplace. Even if it were possible to achieve—and abundant evidence establishes that it is not—this result can be considered socially undesirable.[6] This approach was used, in fact, by industrial engineers in developing classical work measurement theory; it failed precisely because the abilities, needs, and purposes of the individuals and groups involved were not taken into account.

[6] Abruzzi, *ibid.*

Conversely, if one were to consider these problems exclusively in social terms, the goal of prediction would be completely ignored in the interest of giving workers complete freedom in determining how, when, and with what they work. This seems to be the feeling of all too many social scientists, who have made a fetish of flogging modern industrial technology for what it has done for humanity. With naïve wistfulness, they extol the largely non-existent virtues of bygone eras and simple civilizations where—largely because they are bygone—humanity presumably was happily occupied doing exactly as it pleased. Apparently this sort of happiness means doing without adequate food, shelter, material goods, and leisure, which might be considered by some to imply doing nothing as one pleased.

Such extreme viewpoints are the product of the apparently basic human desire to find a simple solution to a complicated problem; they represent a child-like hope that acting as though a problem is simple will make it so. As in the work measurement case, however, the problems in the socio-physical sciences are not simple. Instead, they require a methodology in which a useful degree of precision is attained subject to a well-defined limitation on the number and the degree of restraints imposed on human freedom of action.

The final decision on how such essentially contradictory goals are to be weighted in any given case must ultimately rest with the full social group involved. An important example of this is provided by modern industry where collective bargaining procedures are used to make decisions on problems affecting both the welfare of management and that of labor.

In the main, however, the full social group has merely exercised a permissive role in defining the goals and weights to be used in the socio-physical sciences. It has allowed those considered expert in these fields to make these decisions on the basis of what they think are rational social goals. This situation is understandable in view of the inability of large social groups to understand the implications of what the experts are doing. Also significant here is the refusal or inability of many experts to explain to the society what they are doing, due perhaps to the desire to preserve the standing of their work and the importance of their station.

To correct the situation, the experts in these fields must develop a fuller sense of social responsibility. In particular, since the society in question must rely to a large extent on the opinion of experts, they should clearly state what social and other restraints are involved in attempting to satisfy certain alternative goals. In this way, they can help the society make decisions regarding which

goals are desirable and how they are to be weighted.[7] It then becomes the expert's function to develop a methodology which best satisfies the goals selected.

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SOME FAMOUS GHOSTS IN ETHICAL THEORY

AT the present time those who wish to discuss the problems of ethical theory cannot afford to ignore the obviously popular charges which the logical positivists have leveled against the verifiability of ethical judgments; nor can they in fact afford to agree with the view of these critics, since they would thereby merely confirm the futility and meaninglessness of ethical disputes. We owe to the positivists the important discovery that our impressive collections of ethical theories are seriously defective as they now stand. There simply is no longer any point in adding to the bulk of ethical prejudices without some attention to the question of the nature of valid ethical judgment. Why then does one find that recent statements continue merely to applaud the conclusions of the logical positivists or to bypass this fundamental question?

There seems in fact to be a rather persistent disorder in ethical theory which makes really useful contributions somewhat scarce. I refer to the apparently little-known fact that the entire domain is regularly haunted by some unusually active ghosts. One is of course always, in such matters as this, open to the charge of hallucination. But it would be an issue of general concern if a family of ghosts were actually lurking about. I have seen several "manifestations" myself and the discussions of others interested in ethical theory leave no doubt that they too have unwittingly contacted a few of these distressed spirits. I confess I know absolutely nothing about exorcism but I think I may be able to report on the original manner of death of some apparitions at least. And I strongly recommend that steps be taken to enlist the aid of those specialists who may permanently rid the field of ethics of these troublesome intruders.

The ethical doctrines of the logical positivists—to consider some unfortunate histories—though they were responsible for having reoriented inquiry in the most fruitful direction possible,

[7] The problems involved in attempting to arrive at optimal social decisions are considered in detail in Arrow, Kenneth J., *Social Choice and Individual Values*, John Wiley & Sons, 1951.

may very well have died by deliberate suicide. One may instance the rather violent end of A. J. Ayer's ethical theory, which took its own life apparently in a sudden and exceedingly depressed mood. There is a telltale desperation that appears in a final and plaintive note insisting that "one really never does dispute about questions of value,"[1] that "in so far as statements of value are significant, they are ordinary 'scientific' statements; and . . . in so far as they are not scientific, they are not in the literal sense significant, but are simply expressions of emotion which can be neither true nor false."[2]

Symptomatically, Ayer's account offered not the slightest shred of evidence to support its charge. As a matter of fact, it seems to have insisted on treating the quite substantial problem of ethical judgment as a devilish delusion. But common sense recognizes a difference between disputes centering on the question, What is this thing? and on the question, What kind of involvement with this thing is morally worthwhile? And no amount of debate will dissolve the difference between the two. The diagnosis therefore is difficult to hide: having mastered the procedures for verifying "scientific" statements, Ayer's account sought to apply these directly to ethical judgment; and, having failed, it attempted to deny its reality; but the persistent nature of the moral problem forced itself on the attention of the account at least as a hallucination and finally drove it to utter despair.

The root disorder of the entire point of view seems to have been hidden in the assertion that ethical claims

> . . . are unverifiable for the same reason as a cry of pain or a word of command is unverifiable—because they do not express genuine propositions.[3]

compare his "On the Analysis of Moral Judgments" in *Horizon*, Vol. XX, no. 117, September 1949, pp. 171–184.

For

> . . . in every case in which one would commonly be said to be making an ethical judgment, the function of the relevant ethical word is purely "emotive." It is used to express feeling about certain objects, but not to make any assertion about them.[4]

Ayer's account might easily have faced a rosier future if it had merely acknowledged that, though ethical *claims* may involve emotive expressions, propositions about these expressions are simply ordinary propositions of science, that *ethical judgment* is

[1] A. J. Ayer, *Language, Truth and Logic*, revised edition (New York, n.d.), p. 110.

[2] *Ibid.*, p. 103.

[3] *Ibid.*, pp. 108–109. Ayer has reaffirmed his view in recent years;

[4] *Language, Truth and Logic*, p. 108.

concerned with the status of ethical *claims* and not with *propositions* about alleged matters of fact, and that such judgment does not make use of the emotive expressions normally accompanying ethical claims. Once these distinctions are admitted, the question of the verifiability of ethical claims is not at all obviously settled in the way in which Ayer's account would have us believe. On the contrary, if we are to accept the view of another, and quite prominent, member of the logical positivist movement, there would seem to be no serious reasons for ruling out the possibility of formulating an adequate procedure for validating ethical claims:

> Grammatical rules are not found anywhere in nature, but are made by man and are, in principle, arbitrary; so you cannot give meaning to a sentence by *discovering* a method of verifying it, but only by *stipulating* how it shall be done. Thus logical possibility or impossibility of verification is always *self-imposed.* If we utter a sentence without meaning it is *always our own fault.*[5]

It is perhaps also revealing that the fraternal view of C. L. Stevenson, which is less easily unnerved than that of Ayer, quite freely admitted during its lifetime that normative ethics "deliberately deals with a type of disagreement that science deliberately avoids."[6] Here at least one might reasonably have expected no retreat from the "reality-principle."

And yet, Stevenson's ethics was no less suicidal. For, though the distinction of these issues was admitted, no basis was provided for the settlement of normative debates. This is apparent from the "working models" of ethical judgments which Stevenson supplied:

> (1) 'This is wrong' means *I disapprove of this; do so as well.* (2) 'He ought to do this' means *I disapprove of his leaving this undone; do so as well.* (3) 'This is good' means *I approve of this; do so as well.*[7]

The doctrine went on to explain that the definiens in each case is composed of a declarative statement and an imperative statement.[8] It regarded these models, it is true, as somewhat crudely constructed. But the only corrections intended were those of increased clarity (for the declarative statements) and decreased harshness (for the imperative statements).[9]

[5] Moritz Schlick, "Meaning and Verification," reprinted from *The Philosophical Review* in H. Feigl and W. Sellars, *Readings in Philosophical Analysis* (New York, 1949), p. 155.

[6] C. L. Stevenson, "The Nature of Ethical Disagreement," reprinted in Feigl and Sellars, op. cit., p. 592.

[7] C. L. Stevenson, *Ethics and Language* (New Haven, 1944), p. 21.

[8] *Ibid.*, p. 22.

[9] *Ibid.*, pp. 31–36.

So Stevenson's view seems to have been an unwitting suicide, nicely fitted to the old rule, "One man's meat is another man's poison." It had been nourished exclusively on a diet suited only for rhetoric; and so it perished. I say this a bit uneasily, because there is the barest suspicion that the commitment was wilful, although the intent is not at all clear. There is the disturbing admission that agreement on scientific beliefs does not logically entail ethical agreement.[10] And still the doctrine insisted on limiting its interest precisely to those disputes in which "a commonly accepted body of scientific beliefs would cause us to have a commonly accepted set of attitudes."[11] We may fortunately take refuge in the ambiguous circumstances surrounding its quietus and attribute death to respectable confusion.

It remains, perhaps, to warn others who work with ethical theories that these cannot be kept alive on a diet of distinctions suitable for rhetoric alone. In fact, the problem of ethical judgment will find, contrary to Stevenson's belief, no nutritive value at all in the concept of persuasion. For, if we speak of settling ethical disputes, either there is a criterion to which in principle we may appeal or there is none. In the first instance, persuasion is clearly superfluous to the ethical status of a valid claim; and in the second, distinctly ethical disputes disappear. Or if we speak of persuasion in connection with the acceptability of ethical first principles, the term loses its peculiar relevance for ethics, since it is not an *ethical* problem to determine a cogent ground for ethical first principles.[12]

One wonders whether this suicidal tendency is perhaps a hereditary feature of the entire clan of positivist ethical doctrines. Arthur Pap's account suffers from an ambiguous disorder not unlike that of Stevenson's:

> Consider, then, an ethical sentence like 'disloyalty to your country is evil.' Either the predicate 'evil' has only emotive meaning or it refers to a property. If the former is the case, then the sentence does not assert anything that is either true or false, no more than the imperative sentence,

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[10] "The Nature of Ethical Disagreement," pp. 591–592.

[11] *Ibid.*, p. 591.

[12] There is a very curious oversight in the logical positivist's position which seems to render it, on any view, quite useless. It fails to specify assumptions on which valid persuasion could be publicly recognized, a point of some importance—granting the positivist emphasis on the difference between disputes over matters of fact and matters of value. That is, the assumptions implicit in scientific disputes do not apply as such to ethical disputes; and so Stevenson, for one, cannot really tell when a dispute has been properly settled, *obliging* the participants to commit themselves in accordance with their findings *up to that time.*

which is equivalent in motivative power: 'don't be disloyal to your country!' How, on the other hand, could it be shown that 'evil' has descriptive meaning without postulating, in the manner of the intuitionists, mysterious 'non-natural' qualities apprehended by our moral faculty? Only in a naturalistic manner, i.e. by defining 'evil' as synonymous with some descriptive term, e.g. 'disapproved of by most people.' However, as Stevenson and other adherents of the *emotive theory* rightly contend, such a definition only amounts to a formal disguise of an expression of an attitude: the emotive meaning of 'evil' is utilized, as it were, to direct other people's disapproval towards such behavior as is disliked by the masses, or by the church, etc.[13]

The same melancholic symptoms appear, despite the fact that in an earlier passage Pap's analysis offers the hope of a more promising existence:

To justify a moral rule means to derive it from a moral principle together with an empirical premiss: killing disrupts the peace of a community; disruption of peace is intrinsically bad (i.e. undesirable in itself); hence killing is bad. As we might put it, a moral rule states what is instrumentally good (or bad), a moral principle states what is intrinsically good.[14]

It is necessary to add only that a "moral principle" entails a *non-ethical procedure by which empirical properties may be construed as morally significant*. The problem of ethical theory then becomes the study of the discipline appropriate to this effort, a problem analogous to that of determining the first principles of natural science.[15] There are, by the way, other signs in Pap's account that the old clan pessimism may dissolve itself yet.[16]

Some years ago there appeared a public announcement by a relative or close associate of the positivists declining responsibility for the further mischief of the family's ethical theories.[17] But Henry Leonard (the relative) had, though he quite manfully washed his hands of the black-sheep members of the immediate family who refused to stay decently buried, unfortunately fallen victim to the visitations of more distant cousins. And these, the ghosts of hedonism which he seems to have preferred, both because they are so very hardy and because they appear before non-positivists as well, provide something more than a family squabble. Malicious gossipers are bound to recall, however, that Moritz

[13] Arthur Pap, *Elements of Analytic Philosophy* (New York, 1949), pp. 34–35.

[14] *Ibid.*, p. 26.

[15] This is of course the implication of W. K. Frankena's critique of G. E. Moore's so-called "naturalistic fallacy"; compare his "The Naturalistic Fallacy," reprinted from *Mind* in W. Sellars and J. Hospers, *Readings in Ethical Theory* (New York, 1952), particularly pp. 109–111.

[16] See *Elements of Analytic Philosophy*, pp. 33–35.

[17] Henry Leonard, "Ethical Predicates," this JOURNAL, Vol. XLV, 1948, pp. 281–292.

Schlick, one of the earlier positivists, quite openly admitted his hedonist lineage; they are likely to question how great Henry's change of heart actually was.

All hedonist (in fact, all emotionalist) theories of ethics appear to have died from the effects of an incurable constitutional deficiency. These normally frail natures were, unfortunately, not infrequently burdened to spend their strength in defense of psychological hedonism, that is, the view that men always seek pleasure and avoid pain or that they desire only those objects that are "imagined with pleasure."[18]

The additional effort was not in itself fatal but it served to weaken quite seriously the general condition of these already fragile theories. Moritz Schlick's own account, for example, admitted rather strenuous exertions in behalf of psychological hedonism.[19] Plato long ago had diagnosed the functional difficulties of any hedonist calculus.[20] And the more recent studies of specialists on human motivation are inclined to dispute the value of supporting the thesis altogether. The psychoanalytic studies of J. C. Flügel, for instance, lead to the conclusion that man "might just as truthfully be called a pain-seeking as a pleasure-seeking animal."[21]

It is, however, as an ethical doctrine that hedonism reveals its hereditary failure. Schlick's view, for example, insisted:

> . . . there remains no alternative . . . to finding the verification of a proposition concerning value in the occurrence of a definite experience. Our own criterion is of this sort: the corresponding experience is simply the feeling of pleasure. . . . According to our opinion the *essence* of value is completely exhausted by it.[22]

But it did not have the inherent strength to support this difficult conclusion and it was forced to admit that

> . . . the impulses [emphasizing those toward pleasurable objects] do not form a solid framework of the soul, but constitute rather a plastic mass, which, under the influence of the environment, is constantly changing.[23]

> We see in the dependence of moral valuation upon the state of human society a sure indication that the content of morality is actually determined by society.[24]

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[18] Moritz Schlick, *Problems of Ethics*, translated by David Rynin (New York, 1939), Chapter 3, Section 6.

[19] *Ibid.*, Chapter 6, section 5.

[20] Plato, *Protagoras*, 351–357.

[21] J. C. Flügel, *Man, Morals and Society, a Psycho-analytic Study* (New York, 1945), p. 94; compare the whole of Chapter 7.

[22] *Problems of Ethics*, Chapter 5, section 4.

[23] *Ibid.*, Chapter 8, section 2.

[24] *Ibid.*, Chapter 4, section 4; compare Chapter 6, section 2.

That is, it had to confess that pleasure, far from being a reliable criterion for conduct, is itself quite fluidly associated with a variety of possible commitments following the prevailing social influences.

The very same frailty appeared most strikingly in the hedonism of J. S. Mill, the most earnest member of the entire family. Mill had genuinely considered the problem of the normative education of pleasure preferences. He frankly admitted a discrepancy between what men actually desire and what they ought to desire and a difference between intrinsically superior and inferior pleasures; he acknowledged that men may "pursue" lesser goods though he was not willing to believe that they "voluntarily choose" these.[25]

Of course, as soon as the educability of experiences of pleasure is admitted (and it is difficult to see how it may be profitably denied), the constitutional malady of hedonist ethics is exposed. The infallibility of the pleasure guide cannot be defended and pleasures themselves must be judged by a superior criterion. Hedonism simply died of over-exertion; it was obliged to perform the functions of a full-bodied ethical theory but was organically deficient.

If one grants Schlick's principle of psychological hedonism (which is at least a little dubious), one may say that it is relatively difficult to assign moral obligations or interests which persistently go counter to our general pleasure-seeking tendencies. By this it becomes perfectly clear that hedonism is at best a statement of certain prudential limits within which ethical theories must function.[26] Still, the customary exertions of ethical hedonism remain as murderous as those of Aesop's frog.

Those who have been victimized by the ghosts of positivist ethics and hedonism may find these "obits" rather disrespectful. I can only hope that they will be persuaded that I have acted in the public interest. But I'm afraid I have some additional unpleasantness to mention that may very well try the patience of still other readers. I believe I have discovered some cases in which certain rather rare disorders have proved fatal to other ethical theories (which of course continue to hover about in their

[25] J. S. Mill, *Utilitarianism* (Liberal Arts Press, New York, 1948), pp. 10–11.

[26] The view of E. W. Hall, that Mill regarded pleasurability not as an *ethical criterion* but as an *empirical requirement* which every alleged ethical first principle must fulfill, is an equivalent expression; compare his "The 'Proof' of Utility in Bentham and Mill," *Ethics*, Vol. LX, no. 1, October 1949, p. 9.

ghostly way). In particular, neophobia and teleophobia (though it might hardly have seemed possible) have claimed ethical intuitionism and instrumentalism as their respective victims.

Consider, for example, the case of Dewey's instrumentalism; all of the confirmatory symptoms may be drawn from the following remark:

> The propositions [i.e., propositions of appraisal] refer directly to an *existing* situation and indirectly to a *future* situation which it is intended and desired to produce. . . . When the contexts are taken into account, what emerges are propositions assigning relatively negative value to existing conditions; a comparatively positive value to a prospective set of conditions; and intermediate propositions (which may or may not contain a valuation-expression) intended to evoke activities that will bring about a transformation from one state of affairs to another. There are thus involved (i) aversion to an existing situation and attraction toward a prospective possible situation and (ii) *a specifiable and testable relation between the latter as an end and certain activities as means for accomplishing it.*[27]

The doctrine asserted that a goal is concretely specified and intermediary activities are to be tested to determine their *fitness* for that goal. Thus, by (ii), hypothetical propositions are initially offered which, upon verification, may be recast as statements of fact; by (i) and the intention of the entire account these were to be regarded equally as judgments of value.

Apparently only a class of debates concerned with questions of facts were examined. And this is all the evidence we have of a persistent teleophobia which threatened Dewey's discussion of the means-end continuum.[28] For the account noted only that every end-in-view is to be taken as a means to some other specific end-in-view; that the solution of one problematic situation might prove to be the means for solving another; that activities would be "valuable" if they were so suited that they would solve encountered problems, serving ulteriorly to enable the solution of future problems. The principal emphasis had been upon the *continuum of problem-solving* rather than upon the *worth of the means-end continuum* itself. Nowhere in his *Theory of Valuation* or in other related tracts had Dewey discussed the methods by which the solutions found in (ii) for the problems of (i) might be judged to be morally valuable.

And yet teleophobia remains rampant. It even appears as a complicating factor in the suicide of Stevenson's account where, were it not for the dominant positivist influence, it might easily have been lethal in itself. "It will be convenient," the account

[27] John Dewey, *Theory of Valuation* (Chicago, 1939), p. 13.
[28] *Ibid.*, Chapter VI.

had suggested, "to interchange 'intrinsically' and 'extrinsically' with 'as an end' and 'as a means.'"[29] And it had added the following rules of usage:

> 'I approve of X intrinsically' has the meaning of 'I approve of X when I disregard all of its consequences upon other objects of my attitudes.' 'I approve of X extrinsically' has the meaning of 'The consequences of X meet for the most part with my approval, and so I approve of X when I consider it with exclusive regard to its consequences.'[30]

If one grants the means-end continuum, an isolation of ends from a consideration of means proves untenable. But, though the term *intrinsic* is by this time an unfortunate one, it may be used to refer to the estimate of the *ethical status of a means-end continuum;* while *extrinsic* refers to the *causal structure of that continuum.*[31] The entire weakness of an attack on intrinsic goods[32] (regardless of the alternate weakness of many alleged defenses) rests on a failure to recognize that *the use of the terms "extrinsic" and "intrinsic" may refer to different levels of analysis and judgment rather than merely to different factors on the same level of analysis.* And here we have a clinical solution of the teleophobic tendency.

As it happens, we have at present the spectacle of a related ethical theory struggling with only inadequate power against this disorder. I refer to the views of C. I. Lewis:

> A thing *A* will never be said to have extrinsic value or instrumental value, unless it is meant to imply that there is some other thing, *B*, to which it is or may be instrumental, which has intrinsic value.[33]

Lewis' position wishes to retain a distinction between *intrinsic* and *extrinsic* values, but, though more sympathetic to the discussion of ends than either Dewey's or Stevenson's views, it similarly confines its treatment of them to their role as factors merely in a causal sequence. This is even more emphatically indicated in a later remark:

> . . . all values correctly attributable to objective existents are, in the last analysis, extrinsic only and relative to their possible production of some value capable of being immediately realized in the experience of life.[34]

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[29] *Ethics and Language*, p. 178.

[30] *Ibid.*, p. 177.

[31] This distinction also meets Dewey's objections against the separation of instrumental and intrinsic goods; compare his *Reconstruction in Philosophy* (Mentor Books, New York, 1950), pp. 137–138.

[32] Compare *Ethics and Language*, pp. 175, 181–186, 194–198.

[33] C. I. Lewis, *An Analysis of Knowledge and Valuation* (La Salle, 1946), p. 385.

[34] *Ibid.*, p. 407.

One can only hope that, when the stability of the means-end continuum is assured, attention may be directed to the difference in the procedures required to study the *causal structure* of that continuum and its *ethical status*.

Of the neophobiac intuitionists I shall say very little, since their disorder is the simplest to diagnose. I need refer only to two brief remarks that appear in the account of one of the most prominent of their number, H. A. Prichard:

> The sense of obligation to do, or of the rightness of, an action of a particular kind is absolutely underivative or immediate.[35]
>
> . . . the appreciation of an obligation is, of course, only possible for a developed moral being, and . . . different degrees of development are possible.[36]

If, in the first, immediacy were intended as a *general condition* of moral insight (Prichard had observed that all knowledge is immediate by its very nature[37]), the argument would be a commonplace applicable to all possible theories. But if immediacy be taken as a *criterion* of valid moral insight, intuitionism would not afford any possible basis for mediating between different value claims equally insistent about their immediate certainty. The only confidence which could possibly have supported the latter view would seem to be the naive belief that the system of conventional obligations (whatever they might be) are so ordered as to be permanently adequate and free of internal conflicts. Intuitionism in fact suffers all the ailments of the true neophobic. Apologizing for the ethical *status quo*, it condemns all new and allegedly self-evident obligations as arbitrary. But as an apology it cannot escape the charge of authoritarianism.

In addition, the second remark had admitted in a completely plausible way that the immediate sense of obligation is an insufficient basis for determining moral duties. Intuitionism therefore lay paralyzed between the conviction of the moral worth of certain conventional duties and the fearful knowledge that the sense of duty does not itself suffice to lead us to select these duties; and so it perished.

It is perhaps not altogether unfitting to suggest, in closing, some modest epitaphs that might be set on appropriate stones when our unruly ghosts are finally laid to rest. I offer the following as rather short and to the point:

[35] H. A. Prichard, "Does Moral Philosophy Rest on a Mistake?" in *Moral Obligation, Essays and Lectures* (Oxford, 1949), p. 7.

[36] *Ibid.*, p. 9.

[37] *Ibid.*, p. 15.

Here lies . . .

POSITIVISM, WHO DISTINGUISHED BETWEEN QUESTIONS OF FACT AND QUESTIONS OF WORTH BUT DENIED THAT THE LATTER ARE MEANINGFUL.

HEDONISM, WHO ADMITTED THE MORAL EDUCABILITY OF PLEASURE BUT CONTINUED TO TREAT IT AS AN ADEQUATE CRITERION OF ETHICAL WORTH.

INSTRUMENTALISM, WHO ANALYZED THE CONTINUITY OF THE MEANS-END CONTINUUM BUT REFUSED TO DISCUSS THE QUESTION OF ITS ETHICAL STATUS.

INTUITIONISM, WHO ASSERTED THAT RIGHT MORAL DUTIES ARE IMMEDIATELY GIVEN TO US THOUGH THE SENSE OF OBLIGATION DOES NOT ITSELF LEAD US TO SELECT THEM.

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BOOK REVIEW

Culture; a Critical Review of Concepts and Definitions. A. L. KROEBER and CLYDE KLUCKHOHN, with the assistance of Wayne Untereiner and Appendices by Alfred G. Meyer. (Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. XLVII–No. 1.) Cambridge, Mass: Published by the Museum, 1952. viii, 223 pp. $5.25.

In this book, two leading anthropologists, whose contributions over many years have been characterized by a profound concern for theoretical implications, take stock of what they regard as the central anthropological concept. A wealth of historical and comparative knowledge is brought to bear in describing the rise of of the culture concept, in comments on a comprehensive selection of definitions from all perspectives, and in summaries and conclusions outlining their own theoretical positions. Philosophers concerned with the theory of science will find this a welcome addition to source materials for the social sciences, and useful as a case study in the complexities of the theory of definition.

The historical-dictionary aspect of the work is rich in its offerings. The spotlight falls, of course, on anthropological definitions developing the concept, with Tylor's famous statement of 1871 setting the stage. It becomes perfectly clear that culture is not just a commonplace notion that has always been somehow around the place; it is a surprisingly recent, emerging concept

of the last eighty years which has been extricated with difficulty from its non-scientific relations. Most hopefully, it is cast for the kind of role that energy has in physics or evolution in biology (p. 185). There are many interesting historical pictures in this account—isolated items, such as the particular career of the word *Kultur* in Germany, and profoundly significant comparisons, such as the diversity of relations between the terms culture and civilization. In the latter, it is shown that exactly opposite contrasts have occurred in the use of the terms, and the dichotomy in both cases is seen to reflect "the old spirit-nature or spirit-matter dualism carried over into the field of the growing recognition of culture" (p. 17).

The authors recognize the complex roles that definitions play in scientific procedures, telescoping conceptual and methodological instruments and condensing theories. The distinction between *substantive or descriptive* and *explanatory* definitions is used frequently to disentangle designation of an area and its description from causal accounts. Attention to *functional intent* underlies their major classification of definitions of culture—descriptive, normative (with emphasis on rule or way distinguished from that on ideals or values), psychological (culture as problem-solving, as habit, etc.), structural (pattern-types), genetic. A further type of definition—*epistemological,* in the sense of pointing "to the phenomena and process by which we gain our knowledge of culture" (p. 42)—is specified but not invoked throughout the book. It appears to be meant largely to underscore the view that definitions speaking of culture as a kind of behavior need not be taken to state its essence but only its mode of discovery or, we might add, its mode of verification. The authors' approach thus falls in line with the growing philosophic recognition of the role in science of conceptual constructions not directly or wholly translatable into a set of observable elements or applicable operations. They attribute the lack of clarity and precision in the concept of culture to its lack of moorings in a body of systematic theory (p. 36). Accordingly their review is not intended to culminate in a 165th definition—"it is premature to attempt encapsulation in a brief abstract statement which would include or imply all of the elements that seem to us to be involved" (p. 157) —but to spread out or display the elements themselves. In fact their conclusion is that anthropology has plenty of definitions but too little theory.

The dimensions of evaluation employed in this large enterprise are taken for granted and are not as explicitly formulated as the theory of definition. They can, however, be readily

gathered from the kinds of arguments directed against specific types of or trends in definition. Some are *logical:* for example, certain views are criticized for seeking an explanatory definition without even troubling to find a descriptive one (p. 57). Some are *factual:* for example, culture is found to be only in part an adaptive and adjustive instrument (p. 57); or Freud is found to exaggerate the extent to which cultures deprive the individual over the extent to which they provide gratifications (p. 110). Some are *theoretical* in the sense that a particular part of a definition is referred to a particular theory in a given field. For example, it is noted that the shift in the definitions from the general term "tradition" to the specific term "learning" stems from the growth of learning theory in psychology (p. 45; cf. pp. 57, 154). Or again, it is noted that historical definitions imply "too great stability and too passive a role on the part of man" (p. 49). For the most part, however, such theoretical probing is cast as historical influences in the presentation of a particular view rather than as a critique of assumptions in anthropological application.

A *cultural* component in evaluation is indicated at one point—"The history of the word 'culture' presents many interesting problems in the application of culture theory itself" (p. 35). From this it might seem to follow that a group's view of culture may be in part a significant cultural fact about the group itself. Many items in the historical discussion of the book and in its array of diversities might be cited in support of such an hypothesis. Again, more local *value* elements, stemming from the conflict of schools, seem occasionally to enter—for example, in such remarks as that Radcliffe-Brown "is conducting a rearguard action against the advance of the concept of culture" (p. 133) or that Parsons' system of categories "dismembers the concept of culture" (p. 136)!

The review habit being contagious—and to try out how readily the anthropological issues of this general type could be seen as special cases of significant philosophical problems,—I made the experiment of gathering the references on the *locus* of culture, or what might be regarded as the subject or object of which culture is an attribute.[1] The range is a wide one—from items of behavior at one extreme to eternal objects at the other. In between are individual interactions, face-to-face groups, group

[1] For statements or indications on this question and implied answers, see pp. 85 (Forde), 86 (von Wiese), 87 (Lowie, Murdock), 90 (Kluckhohn and Kelly), 92 (Kroeber, Radcliffe-Brown), 126 (Sapir), 134 (Murdock, Parsons). For the authors on this question, see pp. 148, 155, 172.

trends, process, historical totality, values, meanings; and I have probably overlooked some. No more varied list could have been furnished by Aristotle in his search for what is substance—with Bradley and Dewey as assistants! One view speaks of a natural unit for the ethnologist, another of a most typical social group to support a total culture; and there is even a suggestion that some locus formulations constitute pseudo-dilemmas. The authors themselves state explicitly in discussing values, "The locus or place of residence of values or anything else cultural is in individual persons and nowhere else" (p. 172). But they are ready to exclude behavior as such from culture, giving it the position of a pre-condition of culture (p. 155). Perhaps their clearest answer to the question is seen when they assign to culture the status of "the topmost phenomenal level yet recognized" in the realm of nature (p. 148). While they warn against the dangers of reification in construing culture as an emergent level, they run some risk of incurring the opposite danger of subjectification. But this is mitigated by the suggestion, offered but not explored, that culture is to be seen as a logical construct (p. 189).

This brief comparative experiment, showing the clear parallel between the set of locus alternatives and the variety of philosophical categories, suggests the familiar lesson that the theoretical picture, at least on the more general issues, is not a function solely of anthropological data, but also of philosophical organizing principles. Hence there would be considerable gain for both anthropological and philosophical theory in developing a highly systematic mode of analysis on the evaluative level which would exhibit more fully what anthropological issues are really at stake in the alternative approaches—what theoretical and investigatory interests will be served, what problems will be faced and what tools forged in one way that cannot be in another, and so on. For the anthropologist such inquiry has obvious relevance in the the choice of organizing principles. For the philosopher it provides a testing-ground for the fruitfulness of different formulations which helps make the conflict of older metaphysical and more recent analytical constructions more than a matter of convention. For example, it is worth speculating how different the enterprise in this book might have been in many of its formulations if it had looked not for definitions of culture but for implicit analyses of the expression "x is a cultural phenomenon (fact, event, happening . . .)." To go beyond speculation is a collaborative philosophical-anthropological task involving more than simple juxtaposition of fields. The authors say, "We welcome the participation of philosophers in the problem of what culture is"

(p. 68); but they add (rightly) that help to working social scientists involves real digging into and familiarity with existent usage, not external application of philosophical vocabulary.

There are a number of important questions in the Summary and Conclusion which this review has not touched but which are of definite philosophical interest. Most striking, perhaps, is the way in which Kroeber's historical approach, having shed all sharp dualisms, is no longer seen as antithetical to Kluckhohn's abstractive approach with its recognition of the scientist's constructional creativity. They appear rather as different stages in the growth of a science, or as different estimates of its present position. There are also discussions of the direction in which causality is to be sought, the present status of the question of cultural relativity, and the lines of investigation—those suggested are cultural structures as opposed to content, and individual variability—that are most likely to provide "a dynamic and generalized conceptual model in the area of culture" (p. 181).

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NEW BOOKS

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NOTES AND NEWS

It is with profound sorrow and a very deep sense of loss that we announce the death of Irwin Edman, Johnsonian Professor of Philosophy at Columbia University. He died of a heart attack at his home in New York City on September 4, 1954. He was fifty-seven years old.